

**VII. The Development of the Atomic Theory: (I)
Berthollet's Doctrine of Variable Proportions.**

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Two of the burning questions in science, between the years 1800 and 1810, were the theory of "mixed gases," and the fixed or the variable composition of chemical substances. On each of these questions Claude Louis Berthollet and John Dalton were leaders of thought on opposite sides, and in each case Dalton's ideas were ultimately triumphant. Yet his rights in these directions have never received full attention, the subjects having been treated in an inadequate manner by the historians of science. The present paper shows in outline how the doctrine of constant proportions developed.

The doctrine that chemical compounds have a constant composition is not a discovery made in the XVIIIth century by a certain man. Lavoisier, Wenzel, and Richter were outstanding workers on the subject, and other workers in the same field, and to much the same general effect, were Cavendish, Bergman, Klaproth, Vauquelin, and Kirwan. Many more might be named, for practically all chemists towards the end of the century seem to have judged the doctrine to be a satisfactory account of the facts of chemistry.

But no doctrine can be regarded as established, or even as rightly understood, until it has been called in question and successfully defended. The challenge puts the defenders on their mettle and compels them to con-

sider, perhaps to realise for the first time, precisely what their position is. Such was the case of the doctrine of constant proportion. It seems to have arisen naturally and been taken for granted, assumed as a matter of course, rather than considered and carefully defined. Suddenly, about the beginning of the XIXth century, it was called in question by Berthollet.

Primarily his teaching was concerned with the problems of chemical affinity. He had ample leisure to meditate on the subject during his stay in Egypt, where he had gone as an honoured member of the famous Napoleonic expedition. The first fruits of that leisure appeared in papers, read in Cairo before the shortlived Institute of Egypt in June of the year 1799, in which he propounded the valuable and original ideas of "mass-action" and chemical equilibrium. Berthollet was thus the founder of *chemical statics*. Resuming the subject on his return to France, he developed his ideas into a system of chemistry, which is explained in various Memoirs¹ and above all in his "*Essai de Statique Chimique*," (2 vols., 1803).

His teaching aroused interest everywhere, witness the appearance of English and German translations of his works, and the extent to which his ideas permeated the scientific dissertations² and books of the period. His main contention that chemical change was very largely a matter of two factors, affinity and mass-action, could hardly be denied. The tables of affinity which in the course of the XVIIIth century had become more and

¹ "Recherches sur les lois de l'Affinité," *Mém. de l'Institut*, vol. 3, pp. 1, 207, 228, 1801; vol. 7, 229, 1806.

² Memoirs, in which Proust, Thénard, Gay-Lussac, Avogadro, Berzelius, and Dalton take Berthollet's ideas into consideration will be cited as occasion requires. A supplementary list of memoirs not specially referred to in this paper, will be found in an appendix.

more elaborate, were now seen to be obsolete, for they had been drawn up in ignorance of the effects of mass-action. Torbern Bergman, in particular, had prepared more numerous and more elaborate tables than any that had previously appeared, and had also endeavoured to extend the theory of affinity to the reactions of acids, bases, and salts. Berthollet showed that affinity had nothing to do with these reactions, which are governed by mechanical considerations, such as the insolubility and volatility of a substance. Von Meyer, in his "History of Chemistry," advances the opinion that Berthollet's teaching was neglected. "His principles were held to be totally erroneous. . . . It was thus that Bergman's doctrine, although based upon wrong assumptions, and therefore leading its author to false conclusions, kept for so long a time the upper hand."³ Ladenburg is much nearer the mark when he remarks that "tables of affinity disappear soon after the appearance of Berthollet's 'Statique Chimique.'"⁴ The truth is that Berthollet's ideas supplanted Bergman's with an ease almost unparalleled in the history of science. Karsten remarked in 1803 that not a trace was to be found of our previous ideas on affinity,⁵ and as early as 1801, Fischer, who translated Berthollet's "Recherches" into German, declared that the new view of chemical phenomena was so convincing that it was impossible to uphold the old theory.⁶

³ "Hist. of Chem.," Eng. trans., p. 551, 1906.

⁴ "Hist. of Chem.," Eng. trans., p. 41, 1900.

⁵ *Allg. J. Chem.*, (Scherer), vol. 10, p. 137.

⁶ *Allg. J. Chem.*, (Scherer), vol. 7, pp. 507, 517. It is difficult here to avoid overstating the truth, one way or the other. Of course the disappearance of tables of affinity from the text-books of chemistry, in consequence of Berthollet's teaching, was not absolute. Objections to his teaching were urged by Pfaff in 1811 (see Appendix) and by Davy, "Elements of Chemical Philosophy," pp. 117—124, 1812 (see also Henry, "Elements of Experimental Chemistry," vol. 1, pp. 66—67, 1829). Nevertheless, Berthollet's doctrine of chemical affinity, in its main features, was never seriously challenged.

The time was ripe for a new theory of affinity. That is the explanation of how Berthollet's ideas were taken up so eagerly as was the case. Chemical reactions had been studied in the light of the old theory so thoroughly that numerous anomalies had been discovered, which only the new theory could explain. Even the effect of mass in chemical change had been noted by Bergman. In his "Dissertation on Elective Attractions" (§ 10, 1785) he discusses the reaction, $Ad+c=Ac+d$, where Ac is precipitated. "It now remains to be examined, whether the whole of d can be dislodged by a sufficient quantity of c from its former union. It should be carefully noted in general, that there is occasion for twice, thrice, nay sometimes six times the quantity of the decomponent c , than is necessary for saturating A when uncombined." Bergman noted the effect, but could not explain the principle, of mass-action. That principle, leading straight as it does to the doctrine of chemical equilibrium, was quite foreign to the theory of chemical affinity to which he always adhered.

It seemed to be a necessary consequence of Berthollet's principles that chemical combination takes place in indefinite proportion. He had obliterated the distinction between chemical and physical forces, and regarded solution as produced by affinity between solvent and solute. Hence solutions were compounds.⁷ In the next place, indefinite proportion seemed to be an obvious corollary of his "mass-action" theorem. In any chemical system the state of equilibrium depends on the quantity present of each of the re-agents involved. Hence the larger the amount of a given constituent that might be present, so much the more of this should enter into the composition

⁷ "Essai de Statique chimique," §§ 36, 39.

of the product. So he reasoned. For instance, if hydrochloric acid is added to a solution of copper sulphate in water, the copper is divided between the two acids. It was natural to think that all the hydrochloric acid was combined with its share of the copper, and all the sulphuric acid with its share.⁸

Berthollet's attitude is easily misunderstood. He did not so much contradict as transcend the XVIIIth century view. He did not assert that cases of constant composition were non-existent. He admitted, for instance, those of water and ammonia and the oxides of mercury, and was inclined to think that gases combine in constant proportion.⁹ But he thought these instances arose from exceptional circumstances, which he was perfectly prepared to discuss. His standpoint was that constant composition was the exception, and variable composition the rule.¹⁰

On this matter his great opponent was Joseph Louis Proust. The battle waged chiefly round the oxides of the metals. In the case of a metal which forms more than one oxide, Berthollet held that the oxide at minimum can gradually increase its oxygen without sudden change, till the oxide at maximum is reached. Proust, on his part, while not denying that a metal might yield more than two oxides, directed all his efforts to the study of the two extreme oxides,¹¹ and showed that material of intermediate composition usually consisted of a mere mixture of these two,¹² and

⁸ *Op. cit.*, § 52. But the instance given above is not Berthollet's.

⁹ *Op. cit.*, §§ 206, 207.

¹⁰ *Jour. de Phys.*, vol. 60, p. 347, 1805.

¹¹ *Op. cit.*, vol 63, pp. 438—440, 1806.

¹² *Op. cit.*, vol. 55, p. 331, 1802.

he maintained his point with great conviction and persistence.¹³

The historians of chemistry have not been well inspired in their estimates of the meaning and the result of this controversy.¹⁴ They convey the impression that Berthollet was a person who had "preposterous notions" about the chemical composition of substances and was "deservedly annihilated"¹⁵ by Proust. Ladenburg¹⁶ says the controversy was settled by the year 1809, Kopp¹⁷ and Clarke¹⁸ in 1808,

¹³ Proust's principal memoirs on the subject are :

- (1) Recherches sur le cuivre. *Ann. de Chim.*, vol. 32, pp. 26—54, 1799.
- (2) Sur quelques sulfures métalliques. *Jour. de Phys.*, vol. 53, pp. 89—97, 1801.
- (3) Mémoire pour servir à l'histoire de l'antimoine. *Op. cit.*, vol. 55, pp. 325—344, 1802.
- (4) Sur les sulfures métalliques. *Op. cit.*, vol. 59, pp. 260—265, 1804.
- (5) Sur les sulfures alcalins. *Op. cit.*, vol. 59, pp. 265—273, 1804.
- (6) Sur les oxidations métalliques. *Op. cit.*, vol. 59, pp. 321—343, 1804.
- (7) Sur les muriates de cuivre verd et blanc. *Op. cit.*, vol. 59, pp. 350—354, 1804.
- (8) Faits pour l'histoire du cobalt. *Op. cit.*, vol. 63, pp. 421—442, 1806.

In all probability Berthollet's ideas on affinity and chemical composition were first made known through his lectures at the *École Normale* and the *École Polytechnique*. In 1799, before the "Recherches sur les lois de l'affinité" had appeared, Thénard published a paper in support of Berthollet, and Proust one against him. For Berthollet in reply to Proust, see the "Recherches" and the "Statique Chimique," and also "Observations relatives à differens mémoires de Proust," *Jour. de Phys.*, vol. 60, pp. 284—290, 347—351, 1805; vol. 61, pp. 352—362, 1805.

¹⁴ From these strictures I must except P. J. Hartog, who has given in brief a perfectly just statement of the question at issue, see *Nature*, vol. 50, p. 149, 1894, and also *Brit. Ass. Rep.*, p. 618, 1894.

¹⁵ See Huxley on Descartes and Newton.

¹⁶ "Hist. of Chem.," Eng. trans., p. 45, 1900.

¹⁷ "Geschichte der Chem.," vol. 2, p. 369, 1844.

¹⁸ *Manchester Memoirs*, vol. 47, No. 11, p. 9, 1903.

and E. von Meyer¹⁹ in 1807. A. Wurtz, putting the date even earlier, says "the truth of the fixity of chemical proportion was definitely established in the year 1806."²⁰

These writers do not adduce any evidence in support of their statements. They seem to consider their case so probable that proof is unnecessary. On the contrary their case is not even probable. It depends on a fatal underestimate of the influence of Berthollet. He occupied a commanding position in the world of science, so that his ideas could not fail to receive consideration in full. Not only were his main ideas of the highest intrinsic value but his teaching on the very subject of constant proportion, in the light of the knowledge which was then available, was extremely plausible. There are two reasons for this. First, chemists in the XVIIIth century had concentrated their attention on the outstanding compound of each pair of elements, and on this insufficient basis the doctrine of constant composition had been founded. Berthollet raised a new problem by studying the relation between the different compounds of the same elements. It has already been suggested that his teaching did not so much contradict Lavoisier's and Wenzel's and Richter's as go beyond it. While holding in general that affinity tends to unite substances in all proportions, he pointed out that this tendency could be limited by physical factors such as cohesion and insolubility and elasticity, in which case the compounds would be produced on which the supposition of fixed proportion had been based.

In the second place the wretched state of chemical analysis only too easily afforded data in support of variable proportion. Berthollet's theory suited the existence of discordant analyses of the same substance by

¹⁹ "Hist. of Chem.," Eng. trans., p. 194, 1906.

²⁰ "The Atomic Theory," Eng. trans., p. 9, 1880.

different workers and even by the same worker. He was able to maintain his teaching by quoting able chemists such as Vauquelin and Klaproth, whose results *a priori* were as probable as Proust's.²¹

Proust had to maintain his own analyses in the face of Berthollet's teaching and of the analyses of other chemists. It is quite a mistake to suppose that his results were specially accurate. E. von Meyer surmises that if he had only "calculated the result of his experiments on the composition of binary compounds otherwise than he did, he would have discovered the law of multiple proportions."²² As a matter of fact he frequently expressed his results in a way that must have revealed the law in question, supposing that he had known what to look for, and that his data were approximately correct. For the composition of black oxide of copper he gives copper 100 and oxygen 25, and this is correct, and for the composition of the red oxide copper 100, and oxygen 17—18 instead of 12.5.²³ These figures prove that for the determination of the composition of chemical substances it is not sufficient to have good intentions and a strong conviction that substances are formed in invariable proportions.

In truth the odds against Proust were heavy. He had no principle of the same calibre as the doctrines of mass-action and chemical equilibrium with which to encounter Berthollet. He had to trust to the purely empirical method, and there is no reason to think that it was by means of this method that the doctrine of constant proportion was ultimately established.

²¹ *Jour. de Phys.*, vol. 60, p. 349, 1805.

²² *Op. cit.*, pp. 195—196; see also Arrhenius to the same effect, "Theories of Chemistry," Eng. Trans., p. 16, 1907.

²³ *Journ. de Phys.*, vol. 65, p. 80, 1807.

E. von Meyer says that "none of the other leading chemists of the day raised any objections."²⁴ This puts the matter too strongly, for Thénard, who had previously been on Berthollet's side, showed signs of veering round in 1805. "I am quite persuaded that the number of oxides of the metals is much greater than the majority of chemists allow, . . . but I declare that I am not yet convinced that there are as many oxides as there are possible degrees of oxidation; and if theory allows of them, experience seems to reject them."²⁵ In this paper he maintains that there are not two oxides of iron (as Proust said) but three.²⁶ Proust must have had some adherents, but I do not know of any, unless Thomas Thomson be one,²⁷ who gave him open support. Thénard does not mention him. There is extremely little sign that he was considered to have made out his case. Further, there is every reason to think that the change of opinion, when it came, was due to quite another influence than Proust. What was effective was the working hypothesis which the atomic theory supplied.

The influence of Dalton began to permeate chemistry about the year 1808. For some years he had been making endeavours, not very successful ones, to arouse interest in his theory of chemical combination. In particular, Humphrey Davy,²⁸ with all his powers of imagination, had failed to see anything in it. A much less

²⁴ "Hist. of Chem.," Eng. trans., p. 194, 1906.

²⁵ *Ann. de Chim.*, vol. 56, p. 62, 1805.

²⁶ *Op. cit.*, pp. 66, 77.

²⁷ See *Nicholson's Journ.*, vol. 8, pp. 280—281, 1804.

²⁸ Davy must surely have heard of the atomic theory when Dalton was lecturing at the Royal Institution of London in 1803—1804 (see Henry's "Life of Dalton," pp. 47—50, and Dalton's "New System of Chemical Philosophy," p. v., 1808). He certainly discussed it with Thomas Thomson in 1807 and poured ridicule on it then. (See Thomson's "History of Chemistry," vol. 2, p. 293.)

brilliant man than Davy, Thomas Thomson by name, happening to get an account of the theory from Dalton himself in the year 1804, was wise enough to see its immense importance, and in the year 1807 gave an admirable sketch of it in the 3rd edition of his "System of Chemistry." Then in 1808 Dalton gave his own version of it in the first instalment of his "New System of Chemical Philosophy." Confirmation of the theory had already appeared. At the beginning of the year Thomson had published work on the oxalates of strontium, and William Hyde Wollaston on the carbonates and oxalates of potassium, which they each regarded as exemplifying and justifying Dalton's teaching. This work was of great importance at the moment. As Wollaston remarked afterwards, "Chemists were by no means duly impressed with the importance of this observation of Dalton, until they were in possession of other facts observed by Mr. Thomson and myself."²⁹ The historians of chemistry have failed to perceive the full significance of this work. It refuted Berthollet in a specially telling way, for, in illustrating his doctrine, he had made much use of acid salts of the kind that Thomson and Wollaston examined. He had found them to be of variable composition,³⁰ and now, in the light of Dalton's theory, they were found to be perfectly definite substances.

²⁹ *Phil. Trans.*, p. 6, 1814.

³⁰ "Essai de Statique Chimique," §§ 201—203; *Mém. de l'Institut*, vol. 7, pp. 230—252, 297, 1806. Not only so, but Torbern Bergman ("Dissertation on Elective Attractions," § 9) and J. B. Richter (see report by Karsten of a conversation with Richter, *Allg. J. Chemie*, (Scherer), 10, 138—143, 1803) thought that in many cases salts could be formed with a decided superfluity of either ingredient. Further, it has been shown recently (Joh. D'Ans, *Zeitsch. anorg. Chem.*, 63, 225—229, 1909) that there are four acid sulphates of potassium; hence Berthollet might well think that these salts justified his belief in variable proportion: (see his Intro'uction to Riffault's translation of Thomson's "System of Chemistry," vol. 1, p. 24.)

That Berthollet felt the weight of this refutation of his teaching is shown by the fact that he thought it necessary to repeat Wollaston's experiments.³¹

Further, Gay-Lussac's Memoir on the combining volumes of gases, published in 1809, afforded numerous examples amongst gases of combination in fixed proportion. Berthollet, however, had declared that this was likely to occur amongst gases, a fact which greatly discounted the possible effect on chemists of Gay-Lussac's discovery—of tending to lessen their confidence in Berthollet's doctrine of variable proportion. Besides, at that stage of chemistry, Gay-Lussac was himself reluctant to abandon this doctrine, and still held that, in general, mass-action must produce compounds in all proportions. He maintains the "great chemical law, that whenever two substances are in presence of one another, they act in their sphere of activity according to their masses, and give rise in general to compounds with very variable proportions, unless these proportions are determined by special circumstances."³²

Indeed, Dalton's doctrine of combination in definite and multiple proportions was victorious only in process of time and in consequence of the efforts of J. J. Berzelius. Yet it is worth noticing how much less complete Proust's answer to Berthollet was, than the answer tacitly conveyed by Dalton's doctrine. Berthollet held that affinity tends to combine elements in all proportions, and that the composition of the oxides of a metal at maximum and minimum depended on accidental factors, physical conditions opposed to affinity, such as cohesion and elasticity. Dalton showed that a beautifully simple relation exists between the composition of one oxide and another, so

³¹ *Mém. a'Arceuil*, vol. 2, p. 470, 1809.

³² *Op. cit.*, pp. 232—233.

that the composition is not in the least a matter of chance. Proust had no conception of the law which regulates multiple proportions. Again, Berthollet believed that one oxide could change into the other by continuous variation in composition, while Dalton's theory not only allowed for the existence of definite intermediate oxides, but could even predict their composition with a considerable degree of certainty.

The view taken here, that Berthollet's teaching on the subject of chemical composition easily survived Proust's criticism, and received a serious check from Dalton, can be amply illustrated from the literature of the time. William Henry treats the subject of chemical proportion in that sense. "In opposition to the theory that chemical affinity has a strong tendency to unite bodies in unlimited proportions, an hypothesis has lately been proposed by Mr. Dalton, which appears more consonant to the general simplicity of nature."³³ John Murray pits the two doctrines against one another, and actually, as late as the year 1809, expresses a strong preference for Berthollet's.³⁴ Indeed, Berthollet felt the challenge to himself implied in Dalton's Atomic Theory, and showed this by criticising it in the Introduction which he contributed to the French translation of Thomson's "System of Chemistry,"³⁵ in which, it will be remembered, the theory is sketched.

If anyone, inclining to hold to the view of von Meyer and Wurtz that Proust was successful against Berthollet, should doubt whether the prestige of the latter was so great as has been indicated in this paper, he might do well to consider the chemical literature of the time. In

³³ "Elements of Experimental Chemistry," 6th ed., vol. 1, p. 81, 1810.

³⁴ "System of Chemistry," 2nd ed., vol. 1, p. 627.

³⁵ Riffault's translation, vol. 1. pp. 21—27, 1809.

addition to Gay-Lussac and Murray, who, as already explained, adhered in the year 1809 to the doctrine of combination in variable proportion, Friedrich Stromeyer³⁶ in 1808, and Amadeo Avogadro³⁷ in 1811, showed themselves under the sway and influence of Berthollet. What is more, even after Dalton's doctrine of definite proportion had become the foundation of chemistry, Berthollet's main ideas were still held by chemists in the greatest respect. The very opposite of this might be inferred from E. von Meyer's statement that "his principles were held to be totally erroneous the revival of his principles was reserved for quite modern times,"³⁸ and Sir William Ramsay's conjectural remark that the "*Essai de Statique Chimique*" was soon forgotten."³⁹

Berzelius's "*Essai sur la Théorie des Proportions Chimiques*," published in 1819, contains an exposition of mass-action and chemical equilibrium,⁴⁰ and is dedicated "a l'auteur de '*L'essai de statique chimique*.'" Perhaps the most interesting testimony of this kind is borne by J. B. Dumas, who in his lectures on chemical philosophy, delivered in the year 1836, expressed the highest admiration for the "*Statique Chimique*." "It engrossed my whole time for three or four years; from the age of 17 to 21 I read it, re-read it, and pondered it. . . . I read it pen in hand, making extracts and reflections and comments; these efforts have been of the highest value to me. As a student of chemistry I formed myself on Berthollet and whatever right I have to raise

³⁶ "*Grundriss der Theoretischen Chemie*," pp. 66, 80.

³⁷ *Jour. de Phys.*, vol. 73, p. 76.

³⁸ *Op. cit.*, p. 551.

³⁹ "Introduction to the Study of Physical Chemistry," p. 43, 1904.

⁴⁰ *Op. cit.*, pp 7-11, 106-109.

my voice in this hall I owe it to the study I made of Berthollet's 'Statics.'"⁴¹

Finally, on the establishment of these two doctrines, which seemed to be incompatible with one another—Dalton's of invariable proportions and Berthollet's of mass-action—there remained the problem of reconciling the one with the other. That the necessity of doing this was present in the minds of the leaders of chemistry is proved by Berzelius's "Essay on the cause of Chemical Proportions," etc., which begins with a section "on the relation between Berthollet's theory of affinities and the laws of chemical proportions." He remarks that "some chemists have affirmed that the existence of chemical proportions is contrary to the principles of the theory of affinities with which the illustrious Berthollet has enriched chemistry," and proceeds to show that this is really not the case. He takes the case of solution in water of copper sulphate to which hydrochloric acid is added; ". . . the part of the acid really combined with the oxide is neutralised according to the laws of chemical proportions. . . . This single example is sufficient to show that the principles of Berthollet's theory are not inconsistent with the laws of chemical proportions."⁴²

Dalton, in his comments on this Essay, expresses his full concurrence with the verdict. "The first division of Dr. Berzelius's essay contains an admirable exposition of those facts which Berthollet brought forward in so conspicuous a point of view in his chemical theory, and which his zealous followers have magnified in a still greater degree. A better explanation could, I think, be scarcely given in fewer words."⁴³

⁴¹ "Leçons sur la Philosophie Chimique," pp. 379—380

⁴² *Ann. of Phil.*, vol. 2, p. 443, 1813.

⁴³ *Ann. of Phil.*, vol. 3, p. 174, 1814.

APPENDIX.

List of Memoirs, in addition to those mentioned in the paper, which show Berthollet's influence.

1799.

L. J. THÉNARD, "Les différents états de l'oxide de l'antimoine et ses combinaisons avec l'hydrogène sulfuré," *Ann. de Chim.*, vol. 32, pp. 257-269.

1800.

(A note drawing attention to Berthollet's work on the causes of error in tables of affinity.) *Allg. J. Chemie* (Scherer), vol. 4, pp. 669-670.

1801.

"Ueber den Einfluss einiger unbeachteten Umstände bei der Wirkung der Wahlverwandschaften," *Chem. Ann.* (Crell), vol. 1, pp. 111-114.

1802.

L. J. THÉNARD, "Sur les différentes combinaisons du cobalt avec l'oxygène, etc," *Ann. de Chim.*, vol. 42, pp. 210-219.

1803.

A. B. BERTHOLLET, "Observations sur les précipités des dissolutions métalliques," in "Essai de Statique Chimique," note 22.

E. G. FISCHER, "Vermischte Bemerkungen über die brennbaren Grundstoffe, mit Rücksicht auf Berthollets Theorie der Verwandschaft," *Allg. J. Chem.* (Scherer), vol. 10, pp. 171-184.

L. W. GILBERT, *Ann. der Phys.*, vol. 13, pp. 158-159.

C. J. B. KARSTEN, "Revision der Chemischen Affinitätslehre mit beständiger Rücksicht auf Berthollets neuer Theorie," Leipzig, pp. 278.

L. SCHNAUBERT, "Untersuchung der Verwandschaft der Metalloxyde zu den Säuren. Nach einer Prüfung der neuen Bertholletschen Theorie," Erfurt (for Berthollet's comments on this, see *Ann. de Chim.*, vol. 49, pp. 5-20, 1804).

1804.

GAY-LUSSAC, "Sur les précipitations mutuelles des oxides métalliques," *Ann. de Chim.*, vol. 49, pp. 21-35.

1805.

J.-M. HAUSMANN, "Sur l'oxidation," *Ann. de Chim.*, vol. 56, pp. 5-14.

1807.

C. F. BUCHOLZ, "Die Verhältnismengen der Bestandtheile im salzsauren Silber, und den salzsauren Neutralsalzen," *J. für Chem.* (Gehlen), vol. 3, pp. 328-335.

A. F. GEHLEN [notes accompanying his translation into German of Berthollet's "Troisieme Suite de Recherches sur les lois de l'affinité."] *J. für Chem.* (Gehlen), vol. 3, pp. 248-322.

H. F. LINK, "Ueber Berthollet's Theorie der Chemischen Verwandschaft," *J. für Chem.* (Gehlen), vol. 3, pp. 232-247.

ROSE, "Die Verhältnismengen der Bestandtheile des Schwefelsauren Baryts," *J. für Chem.* (Gehlen), vol. 3, pp. 322-328.

1808.

H. F. LINK, "Einige Bemerkungen über Anziehung und Verwandschaft," *Ann. der Phys.*, vol. 30, pp. 12-22.

1811.

L. W. GILBERT, "Historische-Critische Untersuchung über die festen Mischungs-Verhältnisse in der Chemischen Verbindung, etc.," *Ann. der Phys.*, vol. 39, pp. 361-428.

C. H. PFAFF, "Expériences et observations relatives au nouveau principe d'action de l'affinité établie par M. Berthollet," *Ann. de Chim.*, vol. 77, pp. 259-288 (for Berthollet's comments on this, see *op. cit.*, 288-296).

G. K. L. SIGWART, "Ueber Berthollet's Chemische Masse," *J. für Chem.* (Schweigger), vol. 1, pp. 352-357.

1812.

P. L. DULONG, "Recherches sur la décomposition mutuelle des sels solubles et des sels insolubles," *Ann. de Chim.*, vol. 82, pp. 273-308.

1815.

H. F. LINK, "Ueber die Chemische Wirkung beim Zusammenreiben der Körper," *J. für Chem.* (Schweigger), vol. 14, pp. 193-199.